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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *November* 10, 1865. No. 1.

WARREN DE LA RUE, Esq., President, in the Chair.

Alfred Dawson, Esq., The Cedars, Chiswick;
Charles Stewart, Esq., Akendon House, Alton, Hants, and
L. Barnett Phillips, Esq., 35 Hunter Street, Brunswick Square,

were balloted for and duly elected Fellows of the Society.

On the Comets of 1677 *and* 1683; 1860 *III.*, 1863 *I., and* 1863 *VI.* By M. Hoek.

§ 1. In a former paper I attempted to prove that, before taking their orbits under the influence of the Sun's attractions, the Comets 1860 III, 1863 I., and 1863 VI., formed a system, that is to say, at short distances from each other they had initial movements of the same direction and velocity.

That direction is nearly indicated by the straight line uniting the Sun to γ *Hydri*.

At the end of the same paper I promised to extend my researches on the Comets that appeared before 1844.

The following table contains all the cases since the year 1556, in which the question may arise about a cometary system, in consequence of the successive apparition of comets whose aphelia approach each other on the sphere. In these

investigations I have adopted, as a limit of time the interval of ten years, as a limit of distance the angle of 10°. I confess that there is something arbitrary in these limits, but I have preferred in the beginning not to extend them too much. Moreover, I warn my readers that they may perhaps meet in this table with some combinations in which the distance somewhat surpasses the 10°, because I have only measured these distances on the globe, in order to save the time which would be required for the calculations.

I find:—

Comets.	Direction of Motion.	Aphelion. Long.	Lat.	Notes.
		°	°	
1672	Dir.	279·4	− 69·4	A case analogous to that of the comets of 1845 and 1846. Two retrograde motions, with one direct one. Probably the comet of 1677 is a stranger to a system formed by the two others.
1677	Ret.	286·4	− 75·7	
1683	Ret.	290·8	− 83·0	
1689	Ret.	90·1	+ 0·6	The orbit of the Comet of 1689 is rather uncertain. I have adopted the elements of Vogel.
1698	Ret.	90·8	+ 0·6	
1785 II.	Ret.	67·8	− 52·9	
1790 III.	Ret.	72·5	− 50·7	
1813 II.	Ret.	38·6	+ 24·7	
1822 III.	Ret.	46·2	+ 31·3	
1818 I.	Dir.	273·8	+ 8·4	The orbit of the Comet 1818 I. is rather uncertain.
1818 III.	Ret.	275·4	+ 10·5	
1830 I.	Dir.	31·8	− 2·1	
1835 I.	Ret.	28·0	+ 4·6	
1842 II.	Ret.	181·2	+ 56·6	
1851 IV.	Dir.	193·1	+ 61·2	
1844 II.	Ret.	9·8	+ 22·9	
1845 II.	Dir.	1·9	+ 21·0	
1845 I.	Dir.	280·5	− 41·6	In the § 3 of my former paper I have already indicated that these comets answer only by couples to the fixed limit, and that also they do not answer to the second conditions of having in their orbits a single point of intersection. Probably 1846 V. is a stranger to a system that may have contained the two other bodies.
1846 V.	Ret.	275·3	− 55·4	
1846 VIII.	Dir.	281·0	− 49·5	
1846 VII.	Ret.	340·7	− 28·9	
1847 II.	Ret.	347·4	− 31·7	

Comets.	Direction of Motion.	Aphelion. Long.	Lat.	Notes.
1854 II.	Ret.	347°7	− 76°2	
1858 IV.	Ret.	12·9	− 76·7	
1854 V.	Dir.	345°7	+ 13°5	
1861 III.	Ret.	347°3	+ 18°2	
1855 I.	Ret.	35°0	+ 28·1	
1861 I.	Dir.	36·6	+ 32·9	
1857 III.	Ret.	57°7	− 38·0	
1857 V.	Ret.	53°7	− 42·9	
1857 VI.	Ret.	222·9	− 37°7	
1860 II.	Dir.	219·2	− 29·4	
1860 III.	Dir.	303°1	− 73°2	System the discussion of which is given in the former paper.
1863 I.	Dir.	313°2	− 73°9	
1863 VI.	Dir.	3°3·9	− 76·4	
1862 II.	Ret.	119·6	− 3·6	Combination overlooked in the former paper.
1864 II.	Ret.	124·2	− 0·9	

The harvest is nothing less than rich. To the ten cases belonging to the years 1844–65, the 288 preceding years have only added seven new ones, of which, moreover, two depend on orbits that are less well known. We might have foreseen it. The period 1556–1764 contains in my calculations only 46 comets; that of 1764–1840 only 72 comets; whilst the same number, 72, have appeared in the years 1840–65. Therefore the number of well-observed comets is 0·22 annually in the first period; 0·95 in the second; and 2·9 in the third.

It was an exception when, before 1700, a comet was discovered whose aphelion distance from the Sun surpassed somewhat unity, whilst in the period 1840–65 the number of aphelion distances larger than unity is a third of the whole.

On the one hand, thus astronomers have, by means of their powerful instruments, extended the sphere in which these bodies are detected and observed; on the other hand, the heavens have been explored in the last twenty-five years with a vigilance formerly unknown.

The first-fruit of it has been the discovery of several periodical comets; a second one is the knowledge of the cometary systems.

§ 2. Let us return to our table of concordant aphelia.

How can we distinguish between the cases in which there

are systems and those where there is a fortuitous coincidence? One case only in this table allows of a direct investigation. It is that of the Comets of 1672, 1677, and 1683. First, let us examine if their orbits have a common point of intersection.

The calculation, with Halley's elements, gives—

Comets.	Intersectional Points. Long.	Lat.	
	°	°	
1672 and 1677	275·5	− 72·8	Mean Equinox of 1677·0.
1672 and 1683	236·9	− 82·4	
1677 and 1683	315·9	− 78·8	

and these comets formed thus no system, what we had already presumed from the divergency of their motions.

But how is it with those of 1677 and 1683, that have both a retrograde motion?

We may invoke here a new principle. In my former paper I have indicated that we had commonly to seek for the focal star by which the system was sent to us, in the vicinity of the intersectional point, common to the orbits of all the members of the system. Now, if we presume that two comets have formed a system before approaching the Sun, we must calculate the position of the point of intersection of their orbits; and, if this point coincide with any other known to us as a centre of cometary emanations, we may almost rest assured that these comets formed a system, the origin of which is to be found in the direction of the intersectional point.

It is the case we have here to do with. Let us reduce the point of intersection of the Comets 1677 and 1683 to the mean equinox of 1864·0, and compare it with those belonging to the cometary systems of 1860 and 1863. We obtain—

Comets.	Points of Intersection. Long.	Lat.	
	°	°	
1677 and 1683	318·5	− 78·8	Mean Equinox of 1864·0.
1860 III. and 1863 I.	316·7	− 76·5	
1860 III. and 1863 VI.	312·3	− 75·7	
1863 I. and 1863 VI.	320·8	− 78·7	

After this new coincidence I do not hesitate to express my opinion that in the vicinity of the point

$$\lambda = 319^\circ \qquad \beta = -78^\circ{\cdot}5$$

there must be some star that has sent in the direction of our Sun—first, the comets of 1677 and 1683, secondly, those of 1860 and 1863.

§ 3. In order to justify this opinion, let us make our calcu-

lation of probabilities. When we consider the coincidence of two intersectional points within 2° as a result of chance, its probability is 0·0003. *A priori* we had, therefore, to expect that we should meet with it in a number of 3333 cases, and it occurs in 20 cases.

Moreover, a phenomenon of so small a probability is found to be united to another one that we could as little have had the mathematical expectation of meeting with in the limited number of cases actually considered. I mean the coincidence, within a small circle of a radius of 3°, of the aphelia of three comets that appeared in the course of $3\frac{1}{2}$ years. The probability of this phenomenon being only 0·00000049, we could expect to find it once in 2,050,000 cases, and all our comets furnish only 6,600 cases.

I will pass over in silence the mutual intersection of the same three orbits within a circle of a radius of 1°·5, as well as the limited distance of 2°·5 from the average aphelion to the average intersectional point.

The opinion is already sufficiently justified that the compound event we are considering depends on a physical cause. Is the explanation I have given the right one? Later investigations on the comets that are to appear will decide it. For the present I do not see how to arrive at any other conclusion, and I will proceed to subject the comets of 1677 and 1683 to another trial.

§ 4. Were their distances from the Sun formerly nearly equal?

The formula

$$t = C\,(r + 2q)\sqrt{r-q}$$

with its differential formula

$$dr = \frac{2}{3C}\sqrt{\frac{r-q}{r}}\,dt$$

which suppose a parabolical motion, and in which

$$\log C = 8{\cdot}875232 - 10 \text{ gives the time in years,}$$
$$\log C = 1{\cdot}437812 \quad \text{gives it in days,}$$

give me for the distances expressed in radii of the terrestrial orbit:—

Gregorian Date.	Distance from the Sun. Comet 1677.	Comet 1683.
573·86	600	601·97
837·78	500	502·18
1076·54	400	402·43
1286·93	300	302·89
1464·68	200	203·59
1602·00	100	105·14

and there is no objection, therefore, from that side.*

§ 5. Several questions may arise with respect to the facts I have just proved.

Firstly, there is the point

$$\lambda = 319^\circ \qquad \beta = -78^\circ{\cdot}5$$

whose spherical co-ordinates referred to the equator are

$$\alpha = 4^h\ 3^m{\cdot}5 \qquad \delta = -72^\circ{\cdot}0$$

and which was called the point P′ in my former paper.

We might ask ourselves if there is any interest in searching for a star of well-defined parallax in that direction. As for me I expect that such a star will be found at a few degrees distance from the point P′, for generally we may admit that the stars whence comets come to our Sun are the nearest to us. It would not even be necessary to look for such a star P around the point P′, for it has already been demonstrated in § 7 of my former paper that, in order to come from P′ to P, we must follow on the sphere the average orbit of the comets of 1860 and 1863, and follow it in the direction of the direct movement.† In other terms, and more generally, if we call M the point

* The last table gives an idea of the manner in which the bodies of this system were separated under the influence of the Sun. Perhaps some of my readers like to have before them a similar table for the systems of 1860 and 1863. It is this—

	Distances from the Sun.		
Gregorian Date.	Comet 1860 III.	Comet 1863 I.	Comet 1863 VI.
756·97	600	600·42	600·25
1020·87	500	500·56	500·36
1259·57	400	400·67	400·55
1470·01	300	300·86	300·80
1647·78	200	201·15	201·20
1785·10	100	101·83	102·11
1833·70	50	52·76	53·35
1853·60	20	24·43	25·52
1857·98	10	15·92	17·36

As to both tables I must remark that the distances, calculated in the supposition of parabolical orbits, are only approximate. In order to obtain more correct numbers it would be necessary to make investigations about the excentricity of each of the orbits. As to that point compare § 10.

† I must beg my readers to consider the following part of this paragraph as an erratum on the latter part of the § 7 of my former paper, that is to say, on all what follows in the mentioned § 7, the same words "*in the direction of the direct movement.*" As soon as I detected the error contained in that part I wrote to the Astronomical Society, but it appears that my letter only reached the Society after the printing of my paper.

from which the planetary system removes, P′ P M is a great circle on the sphere, and P lies between P′ and M.

If the point P were discovered we should be able to deduce the velocity of the proper motion of our Sun, on which alone depends the distance P P′.

In order to elucidate this let us suppose that P P′ is found to be 5°, and remember that the proper motion of the Sun, taken in an opposite direction, marks on the sphere the point

$$\lambda = 76^\circ\ 42' \quad \beta = -62^\circ\ 57' \quad \ldots \text{ Mean Equinox of } 1864{\cdot}0.$$

which is distant from P′ by 33° 50′, or, in round numbers, 34°. Now, if we call V the velocity of the comet on its entrance into the sphere of attraction of the Sun, v the velocity of the planetary system, we shall have—

$$v : V = \sin 5^\circ : \sin 29^\circ,$$

or

$$v = 0{\cdot}180\ V;$$

and V itself being 0·367 yearly for an excentricity = 1·001 of orbit of Comet 1860 III., the result is

$$v = 0{\cdot}066 \text{ radii of the Earth's orbit yearly.}$$

I confess that this reasoning is only an example of calculation based on arbitrary suppositions, but it is proper to show the consequences that may be derived from the knowledge of the new facts.

§ 6. We might further put the question if the five comets that were sent to us by this star have left it simultaneously; or, if rather we ought to consider them as despatched at two different times.

It appears difficult to answer that question satisfactorily, but we are able to make researches on the possibility of the circumstances supposed by each of these hypotheses.

Let us, in order to make a trial of the first one, admit—

1. That the parallax of the star is 1″, or its distance 206265 unities.
2. That its attraction becomes imperceptible at a distance of 6265 unities, so that there remained 200,000 unities to be traversed by the comets after their having left the star.
3. That the comets of 1860 and 1863 have left it with velocities that were exactly equal, and of such an amount that the orbit of the Comet of 1860 III. obtains an excentricity of 1·001.

The equations of hyperbolic movement (*Theoria Motus Corporum Cœlestium*, § 21 and 22),

$$u = \frac{\cos\frac{1}{2}(v - \psi)}{\cos\frac{1}{2}(v + \psi)} = \frac{2v}{p}\,\frac{\cos^2\frac{1}{2}(v - \psi)}{\cos\psi},$$

$$\tfrac{1}{2}\lambda e\left(u - \frac{1}{u}\right) - \log u = \frac{\lambda k t}{b^{\frac{3}{2}}}$$

give, for the case of a very large r, the approximate formulæ,

$$u = \frac{2v}{p}\,\frac{\sin^2\psi}{\cos\psi} = \frac{2v}{be}$$

$$\tfrac{1}{2}\lambda e u - \log u = \frac{\lambda k t}{b^{\frac{3}{2}}}$$

or actually,

$$\log u = 3{\cdot}134852 \quad t = 539061 \text{ years.}*$$

$$\text{average velocity} = \frac{200000}{539061} = 0{\cdot}3710 \text{ yearly.}$$

To control this result, let us calculate also the velocity at an infinite distance, given by the formula,

$$v_0 = k\sqrt{\frac{e-1}{q}}$$

in our case 0·001006 daily, or 0·3672 yearly.

In order to allow of the comet's arrival 200 years sooner, it is sufficient that this velocity be increased by its $\frac{1}{2700}$th part, that is, by 0·000000372 unities daily, or by 0·66 metres a second.

As to the divergency of the fragments that arrived successively in 1677 and 1860, let us suppose the Sun to move yearly two unities through space, an estimation that probably is far too high. That body should have traversed, then, during these 180 years a distance of 360, which, seen from the star, represents an arc of 6′ sin 34° or 3′·6.

If, then, the comets of 1677 and 1860 were both fragments of the same body, it would have been sufficient for them to have left the star's sphere of attraction, removed 11 unities from each other, in directions diverging $3\frac{1}{2}$ minutes of angle, and with velocities the difference of which amounts to $\frac{2}{3}$ metre per second.

In the actual state of our knowledge there is therefore nothing absurd in admitting the first hypothesis.

* This result has a very great influence on the reasoning contained in § 5. In an interval of time comparable to such a number of years the Star with well-defined parallax of which there is question in § 5, may have had a very considerable proper motion along the sphere, and be removed far from the point it occupied when the comets started from it. Nevertheless I have preferred to retain that paragraph such as it was written in July, for the reasons mentioned in the note attached to it.

As for the second, according to which we should have received bodies despatched at different times by a same star, it is a question of probabilities.

With a velocity such as probably was nearly that of the Comets of 1860 and 1863, the perihelion distance $q = 1{\cdot}3$ corresponds to such a direction of the initial movement as causes the body to pass the Sun at a distance of 27. When this latter number is doubled the perihelion distance becomes 5·2, that is to say, the comet is no more visible to the inhabitants of the Earth.

We may, therefore, compare the phenomenon to a firing at a target of 120 unities in diameter, at a distance of 206265, and in such circumstances that the person who fires is ignorant in what direction the target is to be met with. Its diameter corresponding to 2′, the probability is only $\frac{1}{4}\sin {}^{2}60''$, that a second shot will strike the target already hit by the first. Firing at random we must discharge 47,300,000 shots in order to have the mathematical expectation 1 of producing the phenomenon.

To come back to our star, even if we knew that it scatters yearly 131,300 comets in space, even then we might *a priori* lay an even wager that the phenomenon of twice striking the solar target, as an effect of chance, will not occur within 180 years.

On the other hand, should there exist a physical cause which compels two successive shots to differ only $3\frac{1}{2}'$ in direction, and, supposing the target to have been already hit, we might lay 1 to 10 that it will be struck again by a second shot, and even 1 to 1 as soon as we know the divergency of the shots, and accordingly the target's motion, to be reduced to a $\frac{1}{8}$th part.

The admission of the second hypothesis includes therefore the supposition of a very great number of comets being yearly hurled into space by the star. Is there anything inadmissible in the number 130,000? If we suppose around the Sun, as a common centre, two spheres, whose radii are unity and *Neptune's* distance, there will be in the larger one a number of perihelia 27,000 times greater than that in the smaller sphere, supposing the perihelia to be uniformly distributed through space. This being so, the smaller sphere contains on an average 2 perihelia yearly, which gives 54,000 comets passing yearly through their perihelia within a sphere large enough to contain our planetary system. Let us add, first, that this number is doubled as soon as we admit that one half of the comets pass without being discovered; secondly, that it seems difficult to reject this uniform repetition of the perihelia, which makes their number proportional to the volume of the sphere, that is, to the cube of its radius.

Finally, neither of the two hypotheses leads us to the admission of anything absurd. It appears, therefore, premature for the present to give the preference to either of them.

§ 7. Let us consider the two hypotheses from another point of view.

If the five comets are fragments of one body, they have been moving towards the Sun in directions diverging by no more than $3\frac{1}{2}'$, and which we may, therefore, consider as parallel to each other. In that case, the five orbits must have a single intersectional point.

On the contrary, if they have been despatched at different times, the intersectional point of the Comets 1677 and 1683 may differ from that belonging to the Comets of 1860 and 1863, and, in that case, a difference between these points of $\frac{1}{2}^\circ$ or even 1° would not at all be astonishing, according to the contents of § 4 of my former paper.

Let us take the numbers from § 2. We have thus the intersectional points

of the Comets of 1677 and 1683, long. $= 318^\circ{\cdot}5$, lat. $= -78^\circ{\cdot}8$
,, 1863 I. and 1863 VI. long. $= 320^\circ{\cdot}8$, lat. $= -78^\circ{\cdot}7$

two points, whose mutual distance is nearly $\frac{1}{2}^\circ$. It is difficult for the present to decide whether we must consider them as two distinct points, or whether we must simply attribute the difference to the lesser certainty of the ancient orbits.

A new computation of these, based on a new and careful reduction of the observations, with exact calculation of the planetary attractions, could alone give us the means of deciding.

§ 8. There remains still the Comet 1860 III., whose orbit passes at a distance of more than $1^\circ{\cdot}5$ the average intersectional point of the four other orbits. The most simple supposition is that this comet has undergone some perturbation. Indeed, I find that, before passing its perihelion, it was near the planet *Mercury*, namely, at a distance of about 0·04. An approximate calculation, however, shows me that the attraction of this planet was insufficient to cause such a notable perturbation in the position of the orbit's plane, in the course of the three or four days during which the two bodies were near each other.

The phenomenon remains, then, unexplained. Has it had its origin near the Sun, or, long before that time, in space? In the latter case, the Comet 1860 III., on coming within the attraction of the Sun, must have had a movement the direction of which was convergent towards the parallel movements of the comets of 1863. May not the mutual attraction of these three bodies have exercised some influence of that kind during the course of several centuries?

§ 9. It is easy to denote their relative position, when at a great distance from the Sun.

First, we have for the mutual inclinations of their orbits,

Inclination of the Orbit 1860 III. on the Orbit 1863 I. = 32° 29′
,, ,, 1860 III. ,, 1863 VI. = 20 27
,, ,, 1863 I. ,, 1863 VI. = 12 1

Further, for the perpendiculars drawn from the Sun on the tangents to the orbits (considered as parabolas), the formula

$$l = \sqrt{r\,q}$$

which gives

	Perpendicular.		
Gregorian Date.	Comet 1860 III.	Comet 1863 I.	Comet 1863 VI.
756·97	13·26	21·85	28 08
1020·87	12·10	19·94	25·63

so that we obtain—

	Date.	
Mutual Distances.	756·97	1020·87.
Comets 1860 III. and 1863 I.	12·81	11·71
Comets 1860 III. and 1863 VI.	16·31	14·90
Comets 1863 I. and 1863 VI.	8·10	7·32

If we prefer to consider the orbits as hyperbolas, we should have to calculate,

$$l = q\sqrt{\frac{1+e}{1-e}}$$

and, therefore, we must admit—

1. That the initial movement is the same for all, or that $\frac{e-1}{q}$ is a constant quantity in the three orbits.
2. That the excentricity of the orbit of 1860 III. has a certain value.

Let this quantity be again 1·001, we obtain then for the perpendiculars,

12·51 21·10 27·13

and for the mutual distances of the comets in space,

12·51 15·86 7·51.

numbers whose proportions do not considerably differ from the above given, and which, should it still be necessary, could give a new proof that before approaching the Sun our comets were near each other in space, moving in equal directions with equal velocities.

Now it follows from these numbers that, if the comets have in space exercised some mutual attraction, we must in the first place seek for the effects of it in the Comets of 1863 which were always the nearest to each other.

No mutual attraction, therefore, explains the deviation of the orbit 1860 III. We might have recourse to the supposition of some meeting with an unknown body that may have more exclusively influenced this comet, but it would be explaining the unknown by the unknown, and that is a way in which I do not like to venture.

§ 10. There is still another circumstance worthy of being mentioned, because it may perhaps lead to a distinction between the two hypotheses of § 6. I mean the distribution of the aphelia around the intersectional points. If we follow the orbits in the direction of the motions of the comets, we meet, in those of 1860 and 1863, with the aphelia before reaching the points of intersection. The contrary is the case with those of 1677 and 1683.

I do not doubt that there is an intimate connection between the excentricity of each orbit and the position of its aphelion with reference to that of the point of intersection. Further, we have a well-known relation between its excentricity and the velocity of its initial movement.

But it seems that any conclusion is premature, before a new computation of the orbits has been made, with an investigation, for each of them, of the maximum as well as the minimum of excentricity, that the observations allow of.

§ 11. Let us return for a moment to the differential formula of § 4.

Neglecting the small quantity q, as we may do in comparison to large values of r, we obtain,

$$dt = \frac{3}{2} C \sqrt{r}\, dr$$

a formula that enables us to deduce the following result:—If we admit that, at a distance of 600 unities from the Sun, the different members of a cometary system may have outrun each other by 10 unities, in consequence of a small difference in their respective velocities, it is possible that three comets, formerly united in a system, may pass through their perihelia during a course of 55 years.

A new combination of our whole stock of aphelion-positions is, therefore, necessary with adequate enlargement of the limit of time, which has been chosen too small in § 1.

I intend to make the investigation.

Utrecht, 8 *July*, 1865.

On the great Sun-spot of October 1865.

By the Rev. F. Howlett.

Many telescopes were, I hope, directed to the remarkable spot which forms the subject of the diagrams now suspended before you.*

The features which this spot exhibited during its development were, I conceive, of rare interest, whilst the definition which prevailed at the time was everything that could be desired.

It was first observed by me on the morning of the 7th Oct. last, at 7 A.M., and when not more than about 3″ from the Sun's eastern margin (fig. 1). It then appeared about 57″ in length from N. to S.; but, in consequence of the extreme effects of foreshortening on the surface of the sphere, only 3″ in breadth from E. to W.

Narrow though the spot, therefore, at this time appeared, yet the positions of the umbræ within it were distinctly visible, and distinguishable from the penumbra in directions of latitude, though I was unable to discern clearly between umbra and penumbra in directions of longitude.

One feature which it struck me as certainly not often to be seen at such an *early* stage of a spot's entry on the disk, was the appearance of two or three luminous patches of faculæ (immediately adjoining the spot on its following side), which extended *completely up to the very margin* of the Sun. It is seldom, I mean, that any difference of luminosity can be perceived in the Solar photosphere, say, within 10″ to 15″ from the limb; though, where faculæ or other inequalities of luminosity exist, they are visible enough when further removed from the margin, their utmost apparent brilliancy manifesting itself, generally, when they are from 20″ to 2′ from it, or thereabouts. By $2^h\ 15^m$ P.M. on the same day (Oct. 7th) the apparent mean breadth of the spot had attained about 7″, and the distance from the limb to the centre of the principal (or western) umbra was 13″ (fig. 2).

The distinction between umbra and penumbra was now in all parts discernible; and I had at this hour more satisfactory evidence of the *shelving* nature of the sides of a Solar spot than I think ever occurred to me before; for the penumbra on the *following* or *outer* side of the umbra was plainly broader by about 3″ than it was on the preceding or inner side, on which last, in fact, it could not have been much more than 1″ in apparent breadth.

So very shallow, however, is the amount always of this shelving, at least in the penumbral strata, and so seldom, comparatively, have I enjoyed the opportunity of observing the entry or departure of a really good-sized spot, furnished at

* These diagrams were exhibited at the Meeting. *Ed.*

the same time with a neat and really *centrally situated* umbra, that it has only been on two or three occasions, during my six years' experience, that I can fairly assert that I have seen this difference of fore-shortening at all. And I cannot but think that some of the supposed instances of the kind have been sometimes fallacious, and that the spots under observation were not really furnished with centrally placed umbræ, and not therefore proper subjects for so delicate an investigation.

On the morning of Oct. 8th the spot had attained a length of about 76″ by an apparent breadth of about 16″. Its centre was now about 52″ from the Sun's limb; and the penumbra appeared equally wide on both the preceding and following sides of the principal umbra, which lay within the southern portion of the spot (see fig. 3).

My observations, however, for 8th Oct. were rather hurried, and I am somewhat inclined to question whether there was really such an absence of umbra in the northern portion, and I should wish my opinion to be checked by that of any other observer.

My notes and drawings for the 10th Oct. are, I trust, as correct as need reasonably be required, so far as the instrument at my command is concerned, aided, moreover, by an atmosphere of unusual serenity (fig. 4).

The centre of the spot (which, it will now be seen, was the leading one of a group) was at 9 A.M. just about 4′ from the limb. The penumbra had a length of about 80″ by a breadth of 50″. The umbra, now of a ragged rectangular form, was situated as nearly as possible in the centre of the spot. It had a mean length of 27″ by a mean breadth of 17″. Two long promontories of different degrees of luminosity extended far across the chasm. The brighter one, 7″ in length (or 3150 miles) from the southern side; the other about 8″ (or 3600 miles) from the northern. As Mr. Brodie observed in a brief notice of this spot to the *Times* newspaper, the shorter and brighter promontory afterwards was separated, and moved away from the penumbra; and the commencement of this operation is plainly visible in my drawing for this day. Not only was the southern promontory the brighter one by far of the two just described, but nearly the whole of the southern side of the umbra was bordered by a luminous band of considerably greater brightness than the penumbral regions immediately adjoining. At the same time, the darkness of tint of the eastern side of the umbra itself was far deeper than that of the western, over which, especially in its north-west angle, floated a feeble luminous haze.

As may be seen in the drawing, the penumbra itself was far from possessing one uniform tint; but, as is often the case, was considerably varied by specks and bands.

In a note for Oct. 11th I have entered the following statement:—"Too stormy to obtain a drawing this day; but about

noon the remarkably luminous band, which bordered the umbra on its south-eastern side, had completely detached itself from the penumbra, and had assumed the appearance of a narrow tortuous 'bridge,' nearly conformable to the wavy edge of the same side of the penumbra; ramifications of feebly luminous matter extending into the northern portions of the penumbra."

On October 12th the great spot had assumed a very different appearance (fig. 5), partly, perhaps, in consequence of the almost entire absence now of the effects of fore-shortening, but even yet more in consequence of the umbra having undoubtedly extended itself much more in an easterly and westerly direction. The great leading spot was now 90″ in its most extreme length by 60″ in mean width. From the north-west side of the umbra projected a long, narrow sigmatoid promontory, somewhat brighter than the adjoining penumbra, about 7000 miles long and 1000 miles wide. Other broader and shorter promontories also stretched into the umbra, whilst a somewhat crescent-shaped and rather less luminous cloud floated as nearly as possible over its centre; some very feebly illuminated matter extending itself in hazy masses 10,000 miles in length over its south-east portions. One or two bright oval patches, from 2000 to 2500 miles in length, lay imbedded in the penumbra, where it was of a somewhat darker hue than the average tint.

By 4 P.M. the sigmatoid promontory had completely detached itself from the penumbra, and had assumed a simple crescent form, *convex* towards the west. Indeed, about one-third of its length at its south-east extremity had disappeared; hence its now crescent shape. Meanwhile, one of the broad southern promontories had nearly united itself to one of the northern ones, curving towards the north-east as it did so. The edge of the penumbra generally was characterised by indentations of considerable depth; but it was still more ragged on the day following.

This is shown in the drawing for Oct. 13th, and is more especially to be observed along its southern border (fig. 6). The remaining outlying and following spots of the group were now arranged after the fashion of a rude triangle, each of whose sides was about a minute and a half in length.

The group this day had attained its most central position on the disk; and as regards size, also, had now reached its utmost dimensions, being about 110″ in length by 60″ still in breadth; and, making every allowance for its oval and also somewhat irregular contour, must have had a superficial area of certainly not less than 972,000,000 square miles. The remaining small spots had a joint area of about 165,000,000 miles; making a grand total of displacement of the solar photosphere to the enormous extent of 1,137,000,000 miles square, or nearly six times that of the whole surface of the earth!

The penumbra on the morning of the 13th was marked by

a long dark streak in its northern portion, about 40″ in length and only about 2″ in breadth, which by noon had become more dark and distinct, as if about to become a narrow umbral *rift;* and other shorter streaks lay in nearly parallel lines with it, towards the south-east; whilst another streak from its north-west extremity ran at right-angles into the northern side of the umbra, and was divided across by a small bright patch at the hour last mentioned; by which time also a large square projecting portion of penumbral matter had nearly detached itself from the main spot towards the south-east.

But the most remarkable feature, perhaps, this day was a bright, sharply-defined arch of photospheric matter, about 9450 miles long in its total curve, which floated over the western side of the umbra, and was united at each extremity to the northern side of the penumbra.

I was now also able, as I thought, to discern four separate nuclei in different parts of the great elongated umbra, which feature itself was not less than 26,000 miles in length by 15,000 in breadth; and wherein, towards the south-east, a large triangular mass of the feebly luminous haze was still plainly observable. It is, perhaps, possible that the arch above described was constructed by the union of one of the promontories with the remains of the sigmatoid extension which existed, as we have seen, on Oct. 12th.

By 8 A.M. on Oct. 14th the great spot presented a very different appearance (fig. 7). The umbra was completely divided across into two unequal portions by an exceedingly bright and nearly straight bridge, 9000 miles long and 1000 miles wide, as before, and which seemed to be formed in part, possibly, out of a modification of the north-west remains of the arch of the day previous. It lay exactly over the principal nucleus of the great umbra, and was considerably dilated at its northern foot. But whether it was that the arch had swung itself loose at its north-east extremity, and then subsequently stretched itself out across the whole width of the umbra, or whether it was an entirely new extension of photospheric matter, I was unable to determine; but subsequent observations have led me to suspect rather the former.

The eastern, and as yet the larger portion of the umbra was now considerably contracted, as regarded its breadth, whilst at the same time the long narrow rift had become more decidedly dark and umbral in its character, in many places. The other streaks which, as we observed above, lay in directions nearly parallel with the rift on the 13th Oct., were now replaced by others, and which, in direct connexion with it, ran off from it into the main umbra, at an angle of somewhere about 45°.

By 2 P.M. a very narrow streak of bright luminous matter extended itself from the northern extremity of the bridge all along the southern edge of the rift,—symptoms of which

formation had manifested themselves as early as 9 A.M. The rift, moreover, was divided by the luminous matter in two or three places, previously to its gradual closing up, as it afterwards proved. Equally curious were certain faint but perhaps not the less important features observable at this time amongst the small subordinate spots that followed in the wake of the principal one. These were evidently diminishing in magnitude, but in their manner of closing up they seemed to betray a noteworthy sort of movement amongst themselves, or perhaps rather, in the first instance, in the photosphere in which they lay scattered. The largest of these minor spots (which was of the dimensions of about 20″ by 15″) had either drifted away from, or had been left behind by, its principal; whilst at the same time a very peculiar sort of *trailing* arrangement was being assumed amongst themselves, indicating a kind of gyratory movement in the photosphere itself. Many of the little spots had ceased to exhibit any appearance of umbra, but, on the other hand, they had become united more intimately one with the other, by means of a wavy and here and there divaricating thread of penumbral specks running through them; an arrangement this, which may also be distinctly observed in my records, for instance, of 11 May, 1863, and of 1 Feb. 1864.

On the 15th Oct. I was unable to make a detailed drawing of the group, but I observed distinctly that the great bridge had again become thoroughly curvilinear in its disposition (fig. 8), extending also further up towards the western end of the umbra, and having a total length of a clear 30″ or 13,500 miles. Indeed, in its now modified condition, and as being almost separated, in its new north-eastern portions, from the adjoining penumbra, it might almost be stated as being even 48″, or 21,600 miles long. The rift was also still plainly to be distinguished, but its more easterly portions had nearly closed up.

On the morning of Oct. 16th the great spot — (or craters, as these phenomena have been called by M. Chacornac, and which term, as Mr. Brodie observed to me, would seem to be a far more appropriate designation for these mighty gulfs in the solar surface) — presented a truly marvellous and most interesting appearance. The great western bridge was now rent entirely asunder. Its northerly half, which remained still connected in that direction with the penumbra, had again become nearly *straight*, and terminated abruptly just above the principal nucleus of the great umbra; whilst its other portion appeared actually to have swung itself round, somewhat after the fashion of a prodigious flying-bridge (*pont-volant*); and, remaining still attached by its extremity to the penumbra, as before, had thrown itself into the form of a huge brilliant *loop;* being exactly a counterpart, in fact, of the curious arch of Oct. 13th; only it now lay on the southern side of the umbra instead of the northern.

As regarded meanwhile the promontories generally, some

of them (especially the most westerly ones) appeared to be obeying some powerful cyclonic impulse, the vortex of which might be assigned probably to the principal nucleus alluded to above. But if so, the impulse in question seemed to be manifesting itself at a lower level than that of the broken luminous bridge, and also of the loop; which last feature had, by noon, this day, very nearly indeed reached to the northern side of the umbra.

The other promontories seemed mostly to have one general tendency of direction towards the west; and the same causes which regulated their shapes and bearings may be seen in the drawings to have for some time past operated conspicuously upon the disposition of the rest of the penumbral matter as well. The subordinate or following spots of the group continued now rapidly to diminish more and more.

Oct. 17th was the last day on which I was enabled to obtain a view of the group, and that was but a comparatively transient one (fig. 10). But the principal spot still appeared about 95″ in length by 65″ in breadth. The large umbra (as I suspected would soon be the case) was now completely divided into two portions by a broad mass of intervening penumbra, dotted in three or four places by dusky patches, indicating the region formerly occupied by the central portions of the one single umbra. At the eastern border of the lately formed penumbra just mentioned a small luminous loop was still observable; and which I rather think was the diminished representative of the striking feature of the previous day, though of this I do not feel quite certain.

At 8 A.M. on the morning of Oct. 20th I observed a somewhat conspicuous and condensed mass of faculæ exceedingly close to the Sun's western limb, and in a latitude which must have very nearly coincided with that occupied by the notable group which had just passed off; and of which, indeed, it was, probably, for the time, the last indications.

On and after Oct. 20th the Sun's disk continued absolutely devoid of spots for fourteen days, till 3d Nov., when one, about 35″ in length, possessing two umbræ, and attended by a fine display of faculæ, made its appearance in nearly precisely the same latitude as that wherein lay the spot which has formed the subject of my paper this evening; and we have, therefore, little room for doubting but what it is the same interesting visitant again come round, and seeking further acquaintance.

I have two drawings of it, on my ordinary scale of one inch to thirty seconds; and in the second of which the faculæ are unusually distinct, and in part, again, coming up absolutely to the very margin of the disk.

With regard, finally, to the prolonged absence of spots alluded to above, I would observe, in conclusion, that I have not known the Sun to be totally devoid of spots for the same

number of days (in the aggregate even) during the last six years; and about one-third of those previous blank days have occurred, be it noted, during about the last six months.

Lastly, then, from all the appearances which have been presented by this remarkable spot we may clearly again infer the fallacy of such theories as would attribute a more or less solid and simply opaque nature to these wonderful phenomena (*especially the darker portions*); be it either that of a cloudy condensation floating above the photosphere, according to some physicists, or of actual crystallization or other form of more decided solidification lying upon it, according to others.

Or how can we suppose that the brilliant and mobile phenomena exhibited by the constantly shifting loops, bridges, promontories, and patches of feebly luminous haze can be rents merely in, or more or less porous portions of, the inspissated matter, disclosing more or less distinctly the subjacent solar photosphere? Would not any such rents, if they were such, prefer to run their course through the less solid portions of the mass—through the penumbræ, for instance, instead of the umbræ chiefly, or, more strangely still, through the nucleus even? But yet, in figs. 5, 6, 7, 8, and 9, it may be observed how the intensely luminous promontory, loop, or bridge (as the case may be), cuts completely through the very *nucleus* itself of the spot: an almost incredible circumstance this, supposing the spots to be solidifications of matter; but perfectly reconcileable with the Herschelian theory (which, in general, is that of nearly all English astronomers who have studied carefully solar phenomena, as well as that of MM. Schwabe, Chacornac, Secchi, and other able continental observers), which assumes, in perfect accordance with the ocular evidence afforded also by the best and most recent methods of scrutinising the solar surface, that the outer portions of the solar orb consist of various strata or envelopes of incandescent liquid or gaseous matter (and in which, possibly, may float particles of a more decidedly solid nature) of different degrees of luminosity, the uppermost being the brightest, and affected by certain most powerful motive forces, the true nature of which is yet enveloped in much obscurity, but which would appear to be, at times, partly cyclonic and perhaps partly also magnetic in their character; and, finally, that the so-called spots or craters are undoubtedly *negative* (that is, cavernous), and not *positive* (that is, concretionary), in their character.

The President said, You will agree with me as to the value of these continuous observations of Mr. Howlett. Unfortunately, members who do not attend our meetings have not the opportunity of seeing how very beautiful are the representa-

tions of Sun-spots, for which we are indebted to Mr. Howlett. It is only by continuity in observations that we can hope to lay the foundation of a theory which will account for all the phenomena of Solar Physics. And while for the time we may accept as useful any theory which will embrace certain phenomena, it should be laid aside as soon as a more comprehensive one is propounded. It is very certain that our business is to multiply accurate observations, and a good result will follow. One thing seems fairly established, namely, that there are several layers of matters in the photosphere, but the relative positions of these strata is still an open question. In a paper now passing through the press by Mr. Stewart, Mr. Loewy, and myself, I think some light will be thrown on this disputed point.

Some Observations on the Solar Craters which appeared on September 28 *and October* 8, 1865. By H. Brodie, Esq.

On September 28 a very fine crater was observed to be on the Sun's disk, the length of the umbra being about 9200 miles. The penumbra was deeply indented at the bottom, where it met the umbra, and one of the promontories formed by the indentations of the penumbra projected much further than the rest; the end of this one was observed to break off in the shape of a roundish mass of luminous matter, having a diameter about equal to the width of the promontory, from which it separated, this nodule of matter was not visible about half an hour later, but seemed to have been diffused on the surface of the umbra, which was covered with a wispy stratum of thin luminous matter, except at one place about the centre of the umbra, where it appeared black, forming the so-called nucleus of the crater. On one side of the umbra a very unusual appearance was noticed; the umbra seemed covered with this luminous matter in the form of what is commonly called mackerel sky, or "cirro-cumulus," the nodular portions of which became smaller as they advanced on to the umbra, and at last diffused into the wispy stratum before mentioned.

September 30. The umbra similar in appearance to that of the 28th inst., the whole being maculated with luminous matter, except the nucleus; these spots of luminous matter being much softened or diffused at their edges. Many nodular portions of luminous matter had broken off from the toothed formation of the penumbra, and were drifting towards the centre of the umbra, but were soon diffused into the thin luminous stratum covering the umbra generally. Some of these toothings, or promontories, were $2''$ of arc long. The penumbra was apparently deeply furrowed from the top edge, or Sun's

surface, to the bottom in tolerably straight and continuous channels, very similar to deep-water courses seen on the sides of a mountain. From watching the effect of these changes it would appear that portions of the photosphere are continually breaking away and sliding down the sides of the penumbra, then breaking off into small round portions and drifting on to the umbra, where they become diffused into a thin luminous stratum; and since this stratum does not become more dense, it must be presumed that this luminous matter forming the Sun's surface is absorbed by chemical actions as yet unknown to us.

October 2. On examining the position of the crater to-day I find that there has been an axial rotation of about $33\frac{1}{2}^{\circ}$ since September 30. This crater disappeared from the Sun's disk about October 5.

October 8. Found a very large crater coming on to the Sun's disk, but too close to edge of disk to examine it well.

October 10. On taking some micrometrical measurements of the crater, and allowing for the foreshortening due to its position on the Sun's sphere, the true shape of the crater appears to be tolerably circular, the umbra having a mean diameter of about 15,000 miles and the penumbra having a mean diameter of about 38,000 miles. There were two long promontories of luminous matter stretching on to the umbra, and nearly opposite to each other, one on each side of the umbra; of these one measured about 3000 miles in length, the other about 4200 miles in length. Soon afterwards the end of the shorter promontory broke off and drifted on to the umbra; about three hours later the whole of this promontory had detached itself from the penumbra, and was being dissipated into thin luminous cloudy stratum.

October 11. The definition this morning is finer than I have yet seen it; used a positive power of 470, with a Dawes' solar eye-piece, and got very sharp definition. The prevalence of passing clouds rendered it impossible to observe continuously. The length of the umbra to-day was about 18,000 miles, the width about 9700 miles. There was an exceedingly long promontory of luminous matter projecting along the umbra from one end of the crater, and running tolerably parallel to one of the sides, and not far from the end of this promontory was another detached mass of luminous matter, about 4000 miles long. (See sketch No. 1.*) This latter mass had elongated itself with wonderful rapidity during the preceding 15 minutes, possibly as much as one quarter of its length. The umbra was covered with the same sort of maculated, or "mackerel-sky" formation of thin luminous stratum, as noticed in the crater of September 28, especially on the opposite side to that near which the promontories of luminous

* The several sketches referred to were exhibited at the Meeting. *Ed.*

matter were located. The very long promontory seemed to be drifting towards the edge of the umbra, while the detached mass was moving away from the edge, and the two masses being so contiguous seemed to argue the existence of a cyclonic action of the disturbing causes.

About 1½ hours later I found that the detached mass had formed a junction with the long promontory (see sketch No. 2), and near the end of this now doubly long promontory there was a curious network sort of formation, consisting of lines of thin luminous cloudy stratum; apparently these had formed by being condensed from the surrounding thin luminous stratum. The prevalence of passing clouds prevented my watching these changes.

October 12. The shape of the umbra very greatly altered, its greatest length this morning was nearly 29,000 miles, while the greatest length of the penumbra was rather more than 50,000 miles. The long promontory of yesterday had entirely disappeared, but there was one at the opposite end of the crater shaped something like an S. (See sketch No. 3.) The singular changes which took place in connexion with this serpentine promontory are shown in sketches No. 3, 4, 5, and 6. The intervals of time being indicated, the outline of the umbra in these last sketches was taken from the image of the crater being thrown by the telescope upon a board. I noticed at one time five nodules of luminous matter that had broken off from the toothings of the penumbra, all in a row drifting towards the middle of the umbra, these were soon after diffused into thin cloudy stratum.

October 13. The sketch No. 7 shows another change from No. 6 that was taken yesterday; the bridge in No. 6 being entirely broken away, and a second umbra is now breaking through the penumbra.

These observations were made with an equatoreal of 8½-in. aperture and 11½ feet focal length.

Molesey Gore, Uckfield, Sussex.

The President desired to make one remark in reference to Mr. Brodie's paper, that one of his observations confirms those of many others, namely, that the luminous matter as it floats across a spot seems to dissolve and disappear.

Some Remarks on the Solar Photosphere.

By Isaac Fletcher, Esq.

Since the date of my communication to the Society on the photosphere of the Sun, printed in the June number of the *Monthly Notices*, I have on very many favourable occasions examined with every possible care and precaution the visible surface of the Sun, employing the same optical means, viz., my $9\frac{1}{2}$-inch refractor, with a Dawes' solar eye-piece and powers from 100 to 550. The result has been a strong confirmation of the opinion I have long held, that recent telescopic observation has (with the single exception of Mr. Dawes' remarkable discovery of the *cloudy stratum*) thrown little, if any, additional light on what Sir William Herschel aptly termed the "Nature of the Sun." Nor, when we reflect on the circumstances of the case, is this to be wondered at, for when that distinguished man observed the Sun he knew better how to proceed to counteract the intense heat of the solar focal point, than to contract the aperture of his telescope, as has been the fashion (until the last few years) in more modern times. In observing the Sun, Sir William placed between the small mirror of his Newtonian reflector and the eye-piece a rectangular vessel with well-polished and parallel glass plates at opposite sides, through which the rays were transmitted on their passage to the eye-piece. By filling this vessel with water, diluted with ink or other coloured liquid, he was able to reduce the light and heat of the solar beam to any amount desired before it was received by the eye, without any contraction of the aperture of the telescope.

Under date, May 3, 1801, using a solution of ink, he says, "Through this mixture I can observe the Sun in the meridian, for any length of time, without danger to the eye or to the glasses, with a mirror of nine inches in diameter, and with the eye-pieces open, as they are used for night observations." (That is without coloured glasses.) With this apparatus there can be no doubt he obtained views of the solar surface equal in every respect to those more recently obtained by means of Sun prisms or other glass diagonals, and his description of the various appearances presented to him corresponds exceedingly well with what I am able to see with my large refractor. I am referring now to the general surface of the Sun, and not to the phenomena visible in the penumbræ of spots. In Mr. Dawes' most interesting paper on this subject, read before the Society, Jan. 8th, 1864, he gives various extracts from Sir W. Herschel's first paper on the "Nature of the Sun," corroborative of this view of the subject; but in Sir William's second paper, printed in the same volume of the *Phil. Trans.* (1801), there are various observations recorded which seem to me conclusive as to the fact of his being perfectly familiar

with the "granules" which have arrested the attention of recent observers, and which some have erroneously supposed to be identical with the "willow-leaf" structure figured by Mr. Nasmyth. I append some extracts from the paper I have alluded to, premising that, beyond all doubt Sir William's "corrugations" are the "rice-grains," "shingle-beach," "minute fragments of porcelain," and "granules" of recent years.

"March 18, 1801. The corrugations all over the Sun are beautiful and coarse, resembling small nodules joined together like irregular honeycomb.

"In a multitude of places the corrugations are quite detached, like luminous wisps, or slender tufts, standing upright.

"March 19. The corrugations are rich, and may be called luminous wisps, being much disjointed except at their bottom; they are so rich that they partake of the yellowish colour of the ridges.

"March 21. At equal distances from the limb the corrugations are equally coarse all over the disk of the Sun.

"March 31. An opening very near the preceding limb is surrounded by a shallow, which is bordered by a luminous ridge all round it. The opening itself is also bordered by an elevated edge, which is nearly as high as the general surface of the corrugations; but not so high as that which borders the shallow, and stands above the general surface.

"April 1. The Sun is now without any openings; but the corrugations are very luminous and rich.

"April 2. The Sun is very rich in luminous corrugations, interspersed with bright nodules towards the south pole.

"April 10. The Sun is full of rich tufted corrugations.

"April 19. The corrugations are extremely rich. The whole solar surface seems to be studded with nodules.

"April 20. The whole surface of the Sun is rich; the corrugations are tufted.

"April 24. The corrugations seem to be closer than they were yesterday.

"April 29. I viewed the Sun through a mixture of ink diluted with water, and filtered through paper. It gave an image as white as snow; and I saw objects very distinctly, without darkening glasses.

"The ridges through this composition appear whiter than the rest of the Sun.

"The tops of the corrugations are whiter than their indentations, instead of approaching to a yellowish cast, as they do in my former way of seeing through green smoked glasses.

"The corrugations are very small and contracted to-day."

From these observations and others in Sir William Herschel's first paper which Mr. Dawes has laid before the Society, and a reference to his drawings, I think that no reasonable doubt can remain on the mind of any dispassionate inquirer, that Sir William's corrugations are identical with the appear-

ances which, under various names, have recently attracted so much attention.

I think it highly probable that from the date of Sir W. Herschel's observations to within the last few years, the Sun has rarely been looked at except with apertures contracted to 2 or $2\frac{1}{2}$ inches, and, under such conditions, the wonderful appearances so graphically described by him are not to be discerned at all, as I have ascertained by direct experiment.

According to the observations of Mr. Nasmyth, the entire photosphere of the Sun is composed of multitudes of lenticular objects, exceedingly regular in form and dimensions, crossing and overlaying each other in every possible direction, and the mottled appearance of the Sun is due to the interstices caused by such interlacing. My observations taken with, I believe, one of the finest telescopes in this country, and under excellent circumstances, not only fail to support Mr. Nasmyth's conclusions, but give direct and strong evidence to the contrary. My conclusion is entirely in accordance with that of Sir W. Herschel and Mr. Dawes; viz., that the "corrugations" or "granules" are portions of the general surface of the photosphere raised high in the outer and non-luminous atmosphere possibly by the furious escape of empyreal gas from the regions beneath the photosphere. I have never seen any appearance whatever indicative of a structure such as is described by Mr. Nasmyth, except what was evidently caused for the moment only by atmospheric disturbance. Nevertheless, I think there is evidence that the photosphere does consist of masses of luminous cloud of some kind, but probably of a substance (so to speak) much more dense and solid than our terrestrial clouds. This, I think, is shown by the manner in which the luminous masses are drawn out and attenuated on the penumbra of the large spots. Here they generally present (though with occasional irregularities, as shown in the beautiful drawings of Mr. De La Rue and Dr. Müller) a perfectly radial appearance, such as one might suppose would be occasioned by a downward rush of the luminous matter on the sudden removal of support from beneath. In such a case the denser portions of the luminous matter and the less dense, would probably so arrange themselves as to produce the beautiful radial appearance in question, whilst lateral disturbance would give the leafy or mossy appearance figured by our President and Dr. Müller.

An apparently fatal objection to the idea of a *downward* rush of the luminous matter is the fact which every observer has noticed, that there is an evident upheaving of the photosphere round the penumbræ of large spots. It seems, therefore, more probable that the appearances on the penumbræ are caused by an *upward* rush of gaseous matter, a theory which, I think, will explain the different appearances surrounding the nuclei of Spots.

After all, it must be confessed that as yet little progress

has been made towards a solution of the query, "What is a Sun?"

Tarnbank Observatory, Cumberland, Nov. 6, 1865.

Mr. Stone: I think in some of his latter letters Mr. Dawes refers to two distinct cloudy strata which he observed on the the solar photosphere, one of which he designates as having coarse and the other finer granulations. I am inclined to think that the coarser of those are identical with the "corrugations" of Herschel. It would appear therefore that Herschel overlooked some of the appearances since described by Mr. Dawes, possibly because his attention was attracted by other phenomena, for example, the dark interstices which he describes as existing between the brighter portions of the photosphere.

The Astronomer Royal suggested that possibly the definition of Herschel's telescope was not so good as those of modern times.

Mr. Brodie: There may be a difference of opinion as to what Herschel meant by corrugations, he may have meant the so-called granules; but there is another very definite feature with large instruments, namely, mountainous ridges—almost like faculæ over the whole of the Sun's surface; these upheavals leaving valleys 60″ or 70″ broad. These ridges of luminous matter are a separate formation from the small granulated appearance of the Sun, and it is possible that Herschel's corrugations are the ridges.

Mr. Stone: I think he refers to these specially under the term of ridges.

Mr. Fletcher: The coarse granules on the Sun may be seen with almost any telescope: one of two inches in aperture will show them distinctly. But it requires a much larger one to see the finer granules on the Sun's disk.

Mr. Stone: May I ask you whether you tried to estimate in any way the dimensions of these granules?

Mr. Fletcher: I think from 2″ to 3″ or 4″.

Mr. Stone: They appear to us to be much smaller.

The President: Certainly they are much larger than the markings which I considered to be Nasmyth's willow-leaves.

Note on an Error of Expression in two Memoirs of the Astronomer Royal, in the Corrections to the Elements of the Moon's Orbit. By the Astronomer Royal.

In a Memoir published in the xviith volume of the *Memoirs* of this Society, page 53, I have used the expression "the

inclination [of the Moon's Orbit to the Ecliptic], whose assumed value is $18537''{\cdot}9$, is to be diminished by $2''{\cdot}44$, and its value is therefore to be reduced to $18535''{\cdot}46$." And in a second Memoir, published in the xxixth volume, page 21, the same form of expression is used, giving $18535''{\cdot}55$ for the inclination.

The words here used are incorrect, as will be seen on reference to the *Reduction of Greenwich Lunar Observations*, vol. i. page lxxix., of which these Memoirs are a sequel. The number $18537''{\cdot}9$ is the coefficient (slightly altered from Damoiseau's $18539''{\cdot}8$) of the first term in the development of the Moon's latitude in periodic functions of the true argument of latitude, and is *not* the inclination. The same remark applies to the numbers $18535''{\cdot}46$ and $18535''{\cdot}55$.

The small corrections made to the coefficient of the first term in the development of latitude correspond without sensible error to the corrections of the assumed inclination.

I am indebted to the careful examination of M. Delaunay, and to his courteous communication, for the discovery of this error.

Royal Observatory, Greenwich, 1865, *Oct.* 23.

On Lambert's Theorem for Elliptic Motion.

By J. J. Sylvester, Esq., F.R.S.

The original demonstration by Lambert of the celebrated theorem which bears his name was a geometrical one, see *Monthly Notices*, vol. xxii., p. 238, where this demonstration is reproduced by Mr. Cayley. Lagrange has given no less than three distinct demonstrations of the same : one a sort of verification by aid of trigonometrical formulæ, another founded on a property of integrals, and a third, perhaps the most remarkable of all, derived from the general expressions for the time in an orbit described about two centres of force varying according to the law of nature by supposing one of them to be situated in the orbit itself, and to become zero. Notwithstandng this plethora of demonstration, the following direct algebraical method of proving from the ordinary formulæ for the time of a planet passing from one point to another, that, when the period is given, the time is a function only of the sum of the distances of these points from the centre of force, and of their distance from one another, may be deemed not wholly undeserving of notice.

Let ϱ, ϱ' be the distances of the two positions from the Sun, c their distance from one another, v, v' the true, u, u' the excentric, m, m' the mean anomalies thereunto corresponding, e the excentricity, $\omega = m - m'$, $s = \varrho + \varrho'$, $\Delta = \frac{1}{2}(s^2 - c^2)$: then

$$\varrho = 1 - e\cos u,\ \varrho' = 1 - e\cos u',\ m = u - e\sin u,\ m' = u' - e\sin u',$$

$$\varrho\cos v = \cos u - e,\quad \varrho\sin v = \sqrt{1-e^2}\sin u,$$

$$\varrho'\cos v' = \cos u' - e,\quad \varrho'\sin v' = \sqrt{1-e^2}\sin u',$$

$$c^2 = \varrho^2 + \varrho'^2 - 2\varrho\varrho'\cos(v'-v).$$

Writing for brevity c, c', s, s', for $\cos u$, $\cos u'$, $\sin u$, $\sin u'$, and to avoid confusion putting also for the moment $\bar{s}$ $\bar{c}$ in place of the original s and c, we have

$$\bar{s} = 2 - ec - ec',\quad \omega = u - u' - es + es',$$

$$\Delta = \varrho\varrho' + \varrho\varrho'\cos(v'-v) = 1 + cc' + ss' - 2e(c+c') + e^2(1 - cc' - ss').$$

Let $J = \dfrac{d(\Delta, \bar{s}, \omega)}{d(e, u, u')}$; then J is the determinant

$$\begin{vmatrix} -2(c+c') + 2e(1+cc'-ss'); & c's - cs' + 2es - e^2(cs' + c's); & cs' - c's + 2es' - e^2(cs' + c's) \\ -c - c' \quad ; & es \quad ; & es' \\ -s + s' \quad ; & 1 - ec \quad ; & -1 + ec' \end{vmatrix}$$

Denoting this determinant by $\begin{vmatrix} A, & B, & C \\ D, & E, & F \\ G, & H, & K \end{vmatrix}$,

we find

$$(A, B, C) - 2H(D, E, F) + 2E(G, H, K) = (0, B, -B),$$
$$(A, B, C) - 2K(E, D, F) + 2F(G, H, H) = (0, -C, C),$$

so that $J = \begin{vmatrix} A, & B, & C \\ 0, & B, & -B \\ 0, & -C, & C \end{vmatrix} = 0.$

Hence restoring s, c, instead of $\bar{s}$, $\bar{c}$, it appears that $d\omega$ is a linear function of ds and $d\nabla$; that is, ω is a function of s and ∇, or what is the same thing of s and c, independent of e. If then, when $e = 1$, the corresponding values of ϱ, ϱ', v, v', u, u' are r, r', θ, θ', ϕ, ϕ', we have $\cos\theta = -1$, $\cos\theta' = -1$, $\sin\theta = 0$, $\sin\theta' = 0$, $r - r' = c$, $r + r' = s$, whence writing

$$1 - \cos\varphi = \frac{s+c}{2},\quad 1 - \cos\varphi' = \frac{s-c}{2},$$

we have finally $\omega = \phi - \phi' - \sin\varphi + \sin\phi'$ as was to be proved.

Essentially this demonstration is of the same value as the first of Lagrange's three methods of proof above referred to, but with the difference that it leads up to and accounts beforehand for the success of the transformations therein employed.

Observations of Encke's Comet. By John Tebbutt, jun., Esq.

I succeeded in discovering Encke's Comet on the evening of the 24th June, with the help of a rough calculation founded on the theoretical elements in No. 1326 of the *Astronomische Nachrichten*, and assuming its perihelion passage to occur on the 1st June. I found it after a short search, but at a considerable distance from its calculated place. It was about two minutes in diameter, faint, and without the slighest condensation of light in the centre. Owing to an unfortunate detention of the mail-steamer, Mr. Farley's Ephemeris did not come to hand till the 30th June. Notice of the discovery was at once forwarded to the Sydney Observatory, and Mr. Smalley succeeded in observing the comet with the 7-inch refractor on the 28th June, and three following days. Owing to bad weather and bright moonlight, Mr. Ellery had not seen the comet up to the 7th instant. It was re-discovered here after the full moon, with the assistance of the ephemeris, but it was so faint as to be hardly distinguishable. The two determinations of position which I send are the best obtained, and may possibly be of some value. I hope to be more fortunate in the observation of Biela's comet, an Ephemeris of which is published in No. 1507 of the *Astronomische Nachrichten*.

I have observations of the variations of η *Argûs* extending from 1854 to the present time, which I will forward at a future opportunity. Should not this star be designated as "Variable" in the *Nautical Almanac?* It is now of the fifth magnitude.

Ring-Micrometer Observations of Encke's Comet.

1865.	Windsor M.T.	App. R.A.	App. Decl. South.
	h m s	h m s	° ′ ″
June 24	6 56 46	7 56 54·8	3 4 36
29	7 5 34	8 35 0·1	13 4 16

The first position is the result of two comparisons with B.A.C. 2660, and the second of three comparisons with B.A.C. 2975.

Mean Places of the Stars of Comparison for the beginning of the Year, and Apparent Places for the Times of Observation.

	R.A. Mean.	R.A. App.	Decl. South. Mean.	Decl. South. App.
	h m s	s	° ′ ″	s
B.A.C. 2660	7 52 59·23	(59·93)	3 18 46·7	(60·7)
2975	8 40 0·02	(0·79)	13 3 15·3	(32·0)

Windsor, New South Wales, 17 July, 1865.

Places of Comet I. 1865 (*Great Southern Comet*), *deduced from Observations made at the Melbourne Observatory.* By R. J. Ellery, Esq.

I forward herewith the final places of the Comet I. 1865, of which I sent you a former notice.

We have only just commenced the zone work, having been much delayed in clearing up all the older observations. Next month I will endeavour to give the Society an account of the method adopted in cataloguing.

Mean Time. Greenwich				R.A.			Log $\frac{p}{P}$	N.P.D.			Log $\frac{q}{P}$	No. of Measures.	Comparison Star.
d	h	m	s	h	m	s	+	°	′	″	+		
Jan. 23	23	5	41·7	21	13	47·07	8·8099	133	10	12·7	9·8378	3	B.A.C. 7439
24	22	46	54·7	21	20	19·26	8·8364	134	19	6·7	9·8047	5	Lac. 8822
25	23	14	26·8	21	27	1·62	8·8229	135	22	40·6	9·8384	6	Lac. 8838
26	22	53	37·9	21	33	22·92	8·8506	136	19	10·2	9·8017	5	* 7th Mag.
27	23	29	5·4	21	39	52·92	8·8273	137	12	48·4	9·8480	4	B.A.C. 7591
30	22	53	17·9	21	58	17·95	8·8831	139	23	52·5	9·7755	3	* 8th Mag.
Feb. 1	23	38	51·1	22	10	28·14	8·8580	140	36	7·0	9·8391	3	Lac. 9084
2	22	55	30·8	22	16	9·47	8·9011	141	6	39·7	9·7621	5	Lac. 9111
3	22	34	56·4	22	21	52·46	8·9176	141	35	8·0	9·7133	4	Lac. 9152
4	22	18	3·5	22	27	32·40	8·9284	142	1	38·9	9·6666	4	B.A.C. 7862
7	22	26	27·2	22	44	16·36	8·9388	143	10	15·3	9·6678	9	Lac. 9280
8	22	25	49·5	22	49	43·37	8·9444	143	29	35·1	9·6600	4	ζ Gruis
9	22	27	14·8	22	55	6·16	8·9458	143	47	35·2	9·6579	3	Lac. 9335
12	23	30	40·4	23	10	59·64	8·9215	144	33	59·6	9·7850	4	B.A.C. 8148
16	22	49	48·8	23	30	51·31	8·9571	145	19	38·2	9·6848	3	B.A.C. 8179
27	23	31	19·8	0	21	9·36	8·9503	146	27	52·3	9·7569	4	* 8th Mag.
Mar. 1	23	35	3·7	0	29	38·44	8·9493	146	34	15·9	9·7626	6	* 8½ Mag.
4	22	24	30·2	0	41	45·57	8·9813	146	41	25·6	9·5777	2	Lac. 182
6	23	16	40·2	0	49	56·43	8·9641	146	45	5·1	9·7202	2	* 7½ Mag.
16	22	39	30·2	1	27	48·28	8·9802	146	51	59·2	9·6275	2	B.A.C. 521
19	22	24	57·9	1	38	32·14	8·9830	146	52	16·1	9·5864	6	Lac. 512

Until February 9th the observations were made with the spider-line micrometer; on February 10th, 13th, and 17th, two

thick silver wires were used, one coinciding with a meridian, the other at an angle of 45° to it. For the remainder of the observations an ordinary wire-micrometer was used, the wires being of thick silver, so as to dispense with lamp illumination. All the comparison stars have been since observed with the transit circle, most of them three times at least, none less than twice. The observations have been corrected for refraction.

Melbourne Observatory, August 25, 1865.

The Astronomer Royal exhibited a Quadrant, the work of Abraham Sharp, lately received at the Royal Observatory, Greenwich, and gave the following account of it :—

The first mention of this Quadrant is in Smeaton's paper in the *Philosophical Transactions*, vol. lxxvi. for 1786, "On the Graduation of Astronomical Instruments;" a paper which, though now entirely superseded as to its fundamental methods by the process introduced by Troughton, is still in many respects worthy the attention of the astronomical-instrument-maker. Speaking of Sharp, he says (page 7), "I believe there is now remaining a quadrant, of 4 or 5 feet radius, framed of wood, but the limb covered with a brass plate; the subdivision being done by diagonals, the lines of which are as finely cut as those upon the quadrants at Greenwich."

In Hutton's *Dictionary*, article "Sharp," this statement (with many others) is copied from Smeaton.

In the *Philosophical Magazine*, vol. xxx. for 1847, January–June, page 25, is a paper by the Rev. N. S. Heineken, "On Abraham Sharp's Mechanical Productions." Numerous instruments are mentioned, and among them, this Quadrant, which Mr. Heineken was so fortunate as to secure from a tinman, who was about to use the material as old brass for the manufacture of kettles.

In the present autumn, Mr. Heineken liberally offered this relic to the Royal Observatory, with the expression of his hope that it might be preserved there with the care usually given to instruments of historical interest. The Astronomer Royal had no difficulty in making this engagement, as far as his personal control could extend; and the instrument, separated into parts as was required for the convenience of carriage, was immediately sent from Mr. Heineken's residence at Sidmouth to Greenwich. Mr. Heineken pointed out that one piece of wood was wanting; this has been supplied at the Royal Observatory, and in this state of comparative completeness the Quadrant was exhibited to the Society.

The wooden frame of the Quadrant consists of three radial bars of oak (of which two are at right angles, and the third,

supplied at Greenwich, makes angles of 45° with the others), and two arch-pieces of oak, one of about 50° and the other of about 60°, whose ends meet in the middle of the quadrant, with mortice-and-tenon connexion, but without any double-curb structure or frame for breaking joint. Upon the three wooden radii are three iron radii and four other iron braces and connecting pieces, screwed to the wood by iron screws tapped into the wood. There is also an iron band (in two pieces, united by an intermediate piece) surrounding the wooden arch; and to this band the brass arch is attached by ten angle-irons. The brass arch itself is made of very thin sheet-brass; it is 10 inches broad in the middle and somewhat broader near the ends. It is composed of four pieces; two of the unions (near the two ends of the arch) are of dovetailed or embattled ends, brazed together (as in the old Greenwich quadrants); the central union, at 45°, does not appear to be brazed; the union seems to be effected by riveting the two parts to an iron plate below. A hole, corresponding to the centre of the graduated circular arcs, was discovered in a small piece of brass which is let into the edge of one of the radial iron bars; this hole is only about $\frac{1}{4}$ inch in diameter; it does not appear ever to have had any centre-work on which an index or telescope could turn. (A small pin is now inserted in it, for convenient lodgment of the point of a measuring beam-compass.) Upon the sheet of brass are several graduated circular arcs, one of which is partly cut away by the form given to the interior edge of the brass sheet; but the finished graduation is a band 1 inch broad very near the exterior edge of the brass sheet; its inner radius being about $4^{ft.}\,11^{in.}$, and its outer radius about $5^{ft.}\,0^{in.}$. Its graduations extend at one end about 3° beyond the quadrant and at the other end about 14° beyond the quadrant. Each degree is divided into twenty parts of 3′ each, at the exterior and interior defining curves of the band; and the graduation-points are connected by diagonal lines. Four intermediate circular arcs divide the breadth of the band into five parts; so that, with a radius properly shaped, an arc would by eye be read to 36″. The diagonal lines are very well cut, but are not (as stated by Smeaton) comparable to those on the Greenwich quadrants.

It is not easy to imagine for what purpose this Quadrant was made. Some bolt-holes in the wooden radii seem to make it probable that it has been fixed to a wall. But there is no appearance that it has ever carried an index or movable radius. The frame, throughout, is miserably weak, and it appears very strange that an experienced workman should have connected such a careful graduation with such a feeble basis, whatever were the purpose for which it was to be used.

Adverting to the occultation-diameter of the Moon as determined in Mr. Oudeman's papers (a determination which, from his personal acquaintance with the author, the Astronomer Royal accepted as worthy of the highest credit), the Astronomer Royal remarked that it was necessary, in comparing different determinations, to keep in view the number of observations on which each was based. The Greenwich determination is founded on the comparison of 295 occultation-diameters with 440 diameters measured by the telescope under the most favourable circumstances; greater numbers probably, in both kinds, than all other observations combined can supply. He remarked that these large numbers were obtained by uniformly acting on the system of examining in each year the observations which it would be possible to make in the next year (in the instance of the telescopic semidiameter, a special calculation being necessary for every day); so that, when the time for observation arrived, every observer and every instrument were in readiness to make the observation.

The President, after tendering the thanks of the Society to the Astronomer Royal for his valuable communication mentioned that the semidiameter of the Moon, which he had been enabled to calculate from photographic observations of the Total Eclipse of 1810, coincided with the Astronomer Royal's. Those which he had selected (without knowing the Astronomer Royal's result) as best representing the observations which he had made concurred absolutely to the second decimal figure with those which the Astronomer Royal had selected as the best, namely, the occultation of a star at the dark limb, and its reappearance at the dark limb of the Moon. Mr. De La Rue, in consequence of the success of this experiment, had very great faith that astronomical photography would give hereafter, in certain classes of observations, very accurate results. In connexion with this subject he also stated that there had been a meeting the preceding day of the Lunar Committee of the British Association, and after much discussion it had been decided to make use of photographs to prepare an accurate outline map of the Moon; and it was intended to distribute portions of these maps to observers who would kindly lend their aid towards producing a more accurate representation of our satellite than existed at present. Following in the same path, he ventured to solicit amateur astronomers to send him an account of their optical means of observation, with the possibility of suggesting a distribution of work. It was proposed that the Moon should be observed in zones of about a degree in width, that every observer would have apportioned to him a zone,

and whenever the Moon was above the horizon some part of the zone would be visible.

Those who had followed lunar photography would be aware that it had its rise in America. Perhaps he (the President) had, by his experiments and the application of time and what talent he possessed, wrested the palm from the Americans; but it was impossible always to hold the sceptre; and he was very glad to be able to say that there were two contributions, one by Mr. Draper, the son of Dr. Draper—a name celebrated in photography—and another by Mr. Rutherford, which surpassed the best which he (the President) had obtained; and he recognised this as a mark of progress in astronomical photography.

The President: I perceive on the table an Ephemeris of Biela's Comet, and I take this opportunity of stating that, having had the advantage of this and earlier ephemerides calculated by Mr. Hind, I have searched diligently, but unsuccessfully, for that comet with my 13-inch reflector.

Mr. Beck presented to the Society a set of stereoscopic views of Lunar Eclipses, by combining pictures obtained by Mr. De La Rue of the eclipses of February 1858 and October 1865.

Elements of Minor Planet (84) *Clio.*

The following set of Elements, calculated by Prof. De Gasparis from observations of August 25 and September 4, 12, and 12, are given *Astron. Nach.* No. 1554:—

Epoch, August 25·49830, Greenwich Mean Time.

Long. of Epoch	=	331 54 41·3	Mean Equinox 15 Sept. 1864.
ϖ	=	338 42 9·4	
Ω	=	327 22 28·9	
i	=	9 23 16·3	
log e	=	9·372048	
log a	=	0·373172	

The latitudes for the first and fourth observations, calculated with the foregoing elements, were

$+0^\circ\ 5'\ 27''\cdot8$ and $+4^\circ\ 21'\ 28''\cdot5$

the observed latitudes being

$+0^\circ\ 5'\ 29''\cdot1$ and $+4^\circ\ 21'\ 30''\cdot6$

Discovery and Elements of Minor Planet (85).

The Planet was discovered by Dr. C. H. F. Peters, at the Hamilton College Observatory, Clinton, N. Y., on the 19th September, being seen as a star of the tenth magnitude. The following set of elements, calculated by Dr. Peters from observations of Sept. 20, 25, and 30, are given *Astron. Nach.* No. 1554:—

Epoch, 1865, Jan. 0, Berlin Mean Time.

			Mean Equinox 1865·0
M_0	=	329° 8′ 28″·6	
π	=	320 34 33·2	
Ω	=	204 55 5·2	
i	=	9 46 33·9	
φ	=	15 20 17·0	
μ	=	824″·104	
$\log a$	=	0·422683	

A number of copies of a paper entitled "Researches on Solar Physics," by Messrs. De La Rue, Stewart, and Loewy, have been placed at the disposal of the Fellows of the Astronomical Society. Those gentlemen desirous of receiving a copy are requested to send their names to the Assistant Secretary.

The President's *soirée* will be held at Willis's Rooms, on Wednesday, the 17th of January, 1866, at nine o'clock; for which cards of invitation are about to be issued. Gentlemen desirous of contributing objects for exhibition are requested to communicate to the Assistant-Secretary the nature of the proposed exhibits.

The attention of the Council has been called to an omission which occurs in their last Annual Report; the signatures of the Auditors ought to have been attached to the revenue account and balances for the year.

The plates containing eight representations of the planet *Mars*, to illustrate Mr. Dawes's paper, Vol. XXV., p. 225, will be issued with the next number of the *Monthly Notices*.

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Printed by STRANGEWAYS and WALDEN, Castle St. Leicester Sq. and Published at the Apartments of the Society, Dec. 12, 1865.